

known species, with notes of their distinguishing characters and geographical distribution. One of the most rare and interesting species figured (in Part 20) is the Teydean Chaffinch, a bird of a blue colour, and which is confined to the upper limits of the pine forests of the Peak of Teneriffe, and to the desolate plains above them, feeding on the seeds of the Retanca (a broom-like plant) and the *Adenocarpus frankenoides*, which characterise those regions, as well as on the seeds of *Pinus canariensis*.

A. R. W.

Lehrbuch der Physik, von Dr. Paul Reis (Dritte Lieferung). Leipzig: 1873.

THIS forms the concluding part of Dr. Reis's useful handbook of Physics. The subject of physiological optics is continued, followed by a description of optical instruments and the laws of the interference and polarisation of light. Heat is treated in the next part, but hardly so fully nor so well as light; radiant heat, for example, occupying less prominence than it deserves. Considerable space is devoted to the explanation of machines for the conversion of heat into motive power: thus we have some of the various forms of steam-engine described, together with a full account of Ericson's heat-engine and Lenoir's gas-engine. Magnetism follows heat, and then we come to static and dynamic electricity and the practical application of electricity. The book closes with a few chapters devoted to the physics of the heavens, or in other words a brief sketch of popular astronomy and meteorology. The principal defect of this handbook is the want of sufficient woodcuts to illustrate the apparatus referred to. The whole work exhibits the characteristic solidity and thoroughness of the German race, and is a marked contrast to some of the recent French popular text-books on Science, the profuse and beautiful illustrations in which almost supplant the letterpress. Let us flatter ourselves that in our nation these complementary races intermingle.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Tyndall and Forbes

It will probably be considered necessary that Dr. Tyndall's pamphlet,* which first appeared as an article in the *Contemporary Review*, be answered at full length. That, however, cannot be decided for some time, as several of those concerned are abroad; but it may be well to let Dr. Tyndall know at once that there is no difficulty whatever in answering him, and that the answer will not lose force or point by a little delay. In the meantime I hope you will give me space to briefly notice a few of the more obvious inconsistencies of Dr. Tyndall's article.

1. Dr. Tyndall is astonished that the "blameless advent" of his "innocent" "modest" "unpretending" volume should be looked upon as reiterating charges made against Forbes. An extract or two will settle this point.

a. "Had he (Rendu) added to his other endowments the practical skill of a land-surveyor, he would now be regarded as the prince of glacialists."

"Professor Forbes, when he began his investigations, was acquainted with the labours of Rendu. In his earliest works upon the Alps he refers to those labours in terms of flattering recognition. But though as a matter of fact Rendu's ideas were there to prompt him, it would be too much to say that he needed their inspiration."

Put these two passages into straightforward English, instead of Dr. Tyndall's favourite style of insinuation, and they amount to this: that Forbes, having the accomplishments of a land-surveyor, and being acquainted with Rendu's work, put this and that together and appropriated the discovery.

b. Forbes had, in 1860, minutely informed Dr. Tyndall of the nature and amount of his knowledge of Rendu in 1842. It

* Principal Forbes and his Biographers.

is not too much to say that Dr. Tyndall's sentence quoted above is utterly inconsistent with the plain statement of Forbes, and so implies a serious personal charge against the latter.

c. A similar serious charge is made, when Dr. Tyndall, knowing that Forbes asserted that it was at his suggestion that Agassiz employed a theodolite or a fixed telescope, and that this had never been denied, carefully states that "the same instrument was employed the same year by the late Principal Forbes upon the Mer de Glace," and that "we are now on the point of seeing such instruments introduced almost simultaneously by M. Agassiz on the glacier of the Unteraar, and by Prof. Forbes on the Mer de Glace."

2. Dr. Tyndall tells us that his work was originally commenced as a boy's book, but that "the incidents of the past year" (i.e. his controversy with Forbes) caused him to deviate from this intention. Have boys so altered since 1859 that such controversy has now become suitable for them when supplied in the "International Series"?

3. What I said with reference to the unpublished correspondence of Forbes was said without any special reference to Dr. Tyndall. It was simply my excuse to the reader for the very meagre use I had made of so extensive and valuable a collection.

But, even in this matter Dr. Tyndall's inconsistency is patent. He says that, longing for peace, he abstained from answering Forbes, not from inability to do so, but to avoid making Science the arena of personal controversy. Yet, in the same breath, he not only complains of my not publishing certain letters which he supposes to contain charges against himself, but (see §5 below) insinuates that I am acting from feelings of animosity!!

4. Dr. Tyndall's answer to one of Forbes' charges is certainly to some extent plausible. I can say no more till I have an opportunity of consulting Rendu, for it is quite obvious that it is possible by proper selection of portions of so vaguely-written a book to make him appear to say anything one chooses.

Dr. Tyndall's answer to the other charge is so obviously insufficient that I need not deal with it here.

But more than this:—no ever-so-complete defence of himself on one or two points is any reply to the overwhelming pamphlet of Forbes, every line of which in its calm truthfulness calls for an answer.

5. Dr. Tyndall refers to former controversy between us, and to its happy termination at a personal interview. Why Dr. Tyndall should bring before the public such matters as a private reconciliation, unless with the object of holding me up to scorn as the breaker of a solemn truce, I altogether fail to see. I need scarcely say that no one in his senses would enter into an agreement never in future to differ from another, nor to point out in his writings passages calculated to mislead. But the following, and other passages which I need not cite, are all so many half-mysterious insinuations (of the Tyndall kind) against me, and all tend towards the same implied accusations.

"... the fire was not extinct: the anger of former combats, which I thought spent, was still potential, and my little book was but the finger which pulled the trigger of an already loaded gun."

I shall be obliged by Dr. Tyndall's pointing out to me a single expression, in that part of Forbes' Life which was written by me, which is calculated to give him the slightest offence:—with the one exception of a letter from Forbes, which was specially written for publication; and which, for Forbes' own sake, I would rather not have published.

No doubt he may be annoyed by my saying that little has since been added to the observations made by Forbes on glaciers. This is a matter of opinion. I do not think that Dr. Tyndall has made any addition of consequence to our knowledge of glaciers, and I am supported in this belief by many of the very highest authorities. But this is no charge against Dr. Tyndall.

6. When I saw the "Forms of Water, &c.," I added a brief and excessively temperate statement to what I had already written, and I republished Forbes' own defence of himself against Tyndall and Agassiz. Was I not bound to do something, and could I possibly have done less?

7. The rupture of the truce, or "peace," whatever that may be, was the work of Dr. Tyndall himself—partly by his "Forms of Water, &c." mainly by his article in the *Contemporary Review*. So far as I am personally concerned, the public has no right to know my feelings:—but, whatever they are, they are mingled with the satisfaction I experience in being once more free, as of old, to point out to the public the misleading passages and actual

errors in Dr. Tyndall's popular works; and to join the too thin ranks of those who, like Mr. Sedley Taylor, are not to be imposed upon by a popular reputation—but venture to think for themselves and to give the public the benefit of the result.

8. Opportunities for such public warning have never been wanting, but now they are so numerous that a long essay would be requisite to do justice to them all.

In the meantime, as an example or two, I may call attention to the way in which Sir Charles Wheatstone, and (by implication) Sir William Thomson, and others, some of whose splendid scientific labours have had the misfortune to become profitable in a pecuniary sense, are treated in Dr. Tyndall's "Lectures on Light," just published. The contrast between the utter contempt for money shown by their censor, and the (implied) opposite which is condemned as unworthy of scientific men, is brought out with all the flow of word-painting and righteous indignation which Dr. Tyndall so abundantly possesses. Besides, the monstrous doctrine is inculcated that men who devote themselves to practical applications are men incapable of original research.

9. But, to conclude for the present, I would simply call attention to the following passage, which comes from an author who in the same work treats of the relative merits of such giants as Young and Fresnel. What confidence can one have in the accuracy of any statement on a scientific matter made by the author of it?—

"And here we may devote a moment to a question which has often been the subject of public discussion—whether, namely, a rainbow which spans a tranquil sheet of water is ever seen reflected in the water? Supposing you cut an arch out of paste-board, of the apparent width of the rainbow, and paint upon it the colours of the bow; such a painted arch, spanning still water, would, if not too distant, undoubtedly be seen reflected in the water. The coloured rays from such an arch would be emitted in all directions, those striking the water at the proper angle, and reflected to the eye, giving the image of the arch. But the rays effective in the rainbow are emitted only in the direction fixed by the angle of 41° . Those rays, therefore, which are scattered from the drops upon the water, do not carry along with them the necessary condition of parallelism; and, hence, though the cloud on which the bow is painted may be reflected from the water, we can have no reflection of the bow itself."—"Lectures on Light," p. 25.

If Dr. Tyndall, with the assistance of his scientific advisers, fails to see the justice of my remark on this passage, perhaps you will permit me to make it the text of a little essay in a future number.

I have all along said, and still say, that I cordially recognise the services of Dr. Tyndall in popularising certain parts of Science. But his readers must be cautioned against accepting as correct great parts of what he has written. It is granted to very few men to do this useful work without thereby losing their claim to scientific authority. Dr. Tyndall has, in fact, martyred his scientific authority by deservedly winning distinction in the popular field. One learns too late that he cannot "make the best of both worlds."

I would request Dr. Tyndall for his own sake, not for mine, should he favour me with a reply, not to pick out one or two isolated passages of a letter, which absence from books may possibly have rendered slightly inaccurate—but to answer me, as he has not answered Forbes, in the full spirit and not in the partial letter.

St. Andrews, Aug. 20

P. G. TAIT

W. S. J. on Hegel

I RESPECTFULLY request admission, into an early number of NATURE, for an explanatory word or two, in reference to W. S. J.'s review of my poor book on Law, &c., in the valuable publication named, for July 24, 1873.

W. S. J.'s very first sentence speaks of the said little book as containing "a discussion of Hegel's opinions concerning gravitation and the differential calculus." In the first place, Hegel has nothing to say against either gravitation as a fact, or the differential calculus as an established method of indubitable scientific calculation: he would only attempt to philosophise both by placing metaphysical principles under them. Now this is part of Newton's own action, and he certainly would not object to any attempt, Hegel's or other, in the same direction. In the second place, I discuss no opinion of Hegel in this reference: I only attempt to expose erroneous opinions of Hegel's relative

opinions. To this I strictly confine myself, and this goes much deeper than the reader may, at first, think.

On Law, whatever is said by W. S. J., concerns only the old difficulty of Hegel's *dialectic*; and perhaps the italicising of this word, together with my own intellectual deficiencies, may be respectfully offered in explanation of as much. W. S. J. here, then, is evidently misintelligent himself, and, accordingly, only speaks so as to induce misintelligence in others. Nevertheless, it is worth saying that the reader may or may not gain from the particularity of satire in W. S. J.'s hands—so keen is it that it cows, and, again, so kindly that it disconcerts.

Mathematically, according to W. S. J., "the critical statement of the necessary outcome of Hegel's philosophy," reduces itself to this, that the principle in question is placed "in that in which the quantum has disappeared, and there remains the relation only as qualitative relation of quantity." W. S. J. has for this only the mildly-authoritative contempt of a duly-elevated position; and when it is said "What is called infinitely little is only qualitative, and is neither little nor great, nor quantitative at all," he at once squelches all by an "on the contrary!" Now all this condemned matter comes directly, not from Hegel, but from Newton; for the former, quoting from the latter, says:—

"These (N.'s increments and decrements) are not to be taken as particles of a definite magnitude (*particulæ finitæ*). Such were not themselves moments, but magnitudes, generated out of moments; what is to be understood, rather, is the principles or beginnings (elements) of finite magnitude;" that is, plainly, what is concerned lies "in that in which the quantum has disappeared as quantum, and there remains the relation only as qualitative relation of quantity."

What concerns comets is naively amusing. We have not had to wait in their regard (as W. S. J. seems to think) for the information of "Chambers' Handbook." The astronomers of the last century, as it appears, were able to speak better than even the "Handbook." Comets that return, they say, though after a great many years, travel in ellipses of enormous axes; whereas those that do not return may describe parabolas or hyperbolas. Such is the opinion of Science yet, though it may talk of many other explanations of non-return, as dissipation, interception, &c. This, I say, is how Science looks yet; but W. S. J., for his part, is under the belief that Science has actually within its ken comets that (so to speak) *revolve* in hyperbolas, as well as others that revolve in ellipses. (Positively such seems his idea. Now, Hegel is never once at fault here—in his own way, I mean; for whether in ellipse, in parabola, or hyperbola, Hegel's assignation of the moment of singularity to the comet is, on his own principles, justifiable. May not a non-returning comet, too, be attributed to that contingency which is, and must be, inherent in externality as externality? On the whole, it may be well for us all to let comets alone yet. Our greatest living authority can only philosophise them into stone-rattles which the sun (for his amusement?) whirls about his head.

One has only to consider these things and others the like—the exquisite little gibes, not forgotten, about a secret in two volumes and a secret in fifteen pages, &c.—to perceive that what we have here is only once again the blind rush of prejudice from its usual dark corner of relative ignorance—an ignorance which it will persist in, and not (through study) convert into the light of day. There is that approbative allusion to Mr. Smith, too; W. S. J. will yet be ashamed of that.

On the whole, however, I hope I have not spoken disrespectfully, for I cannot fancy who W. S. J. may be. He talks of law and logic, and is possibly a lawyer; he certainly has a profound contempt for "Hegel and his satellite Stirling;" but were he (what his initials may indicate) "the eminent Jagers" himself, I cannot, whatever his power of *practise*, admire his capacity for *principles*.

Edinburgh, July 28

J. HUTCHISON STIRLING

Lakes with two Outfalls

IN NATURE, Aug. 14, a paper under this heading concludes thus:—"Colonel George Greenwood, who is, I presume, the same as the former active correspondent about this subject, visited this lake (Lesjeskaugen) last summer, as appears from the entry of his name in the day books. I am not aware that he has since published any opinion, but the lake seems, so far as I can judge, to support his view of the matter.—W. Stanley Jevons." I sent an account of my visit to Lesjeskaugen Lake